

DOCUMENT RESUME

ED 099 302

SO 008 010

AUTHOR Mackie, Robert R.
 TITLE Chuckholes in the Bumpy Road from Research to Application.
 PUB DATE Aug 74
 NOTE 20p.; Paper presented at the Annual Meeting of the American Psychological Association (New Orleans, Louisiana, August 1974)
 EDRS PRICE MF-\$0.75 HC-\$1.50 PLUS POSTAGE
 DESCRIPTORS Communication (Thought Transfer); Curriculum Development; *Educational Development; Educational Research; Educational Technology; *Information Utilization; Learning Processes; *Psychological Studies; *Relevance (Education); Research Design; Research Reviews (Publications); *Research Utilization
 IDENTIFIERS *Social Science Research

ABSTRACT

Neglected activities in the research-to-application process and various characteristics of basic studies in psychology are the causes of failure in translating research into operational applications. A model of activities involved in the research application process includes two areas of strength: (1) basic research on a constructed theory and (2) the development of technology and its practical application. Collation and interpretation of research--a demanding task--and the function of translating, or facilitating communication between research communities and the world of practice, are intermediating activities that need to be emphasized and recognized. The second cause of failure was conceptualized during an investigation of learning research: characteristics of experimental procedure found to determine a study's application potential became more applicable as experimenter control declined. For instance, an artificially controlled environment makes a study's application less practicable than a natural one. A similar continuum operates on the objective, task, stimulus, response, motivation, and time elements of basic research. One way to make basic research more applicable would be to define an investigatable problem through an analysis of an operational problem rather than through a review of prior basic research or through theory alone. (JH)

ED 099302

CHUCKHOLES IN THE BUMPY ROAD
FROM RESEARCH TO APPLICATION*

by

Robert R. Mackie, Ph.D.
Human Factors Research, Incorporated
Santa Barbara Research Park
Goleta, California 93017

U.S. DEPARTMENT OF HEALTH,
EDUCATION & WELFARE
NATIONAL INSTITUTE OF
EDUCATION
THIS DOCUMENT HAS BEEN REPRO-
DUCED EXACTLY AS RECEIVED FROM
THE PERSON OR ORGANIZATION ORIGIN-
ATING IT. POINTS OF VIEW OR OPINIONS
STATED DO NOT NECESSARILY REPRESENT
OFFICIAL NATIONAL INSTITUTE OF
EDUCATION POSITION OR POLICY.

S 800 8010

Several years ago, we were approached by representatives of the Psychological Sciences Division of the Office of Naval Research with a somewhat flattering invitation to try to develop practical innovations from a sample of research studies the Navy had previously sponsored. We were issued the invitation, I believe, because several of our own research studies had led to new procedures and new equipment designs adopted by the Navy. We had not thought very much about *why* some of our research had changed the way the Navy does things and were inclined to feel that we didn't really do research any differently from anyone else. Our immediate assumption was that most research could find application if a modest amount of effort were spent on that objective. Operating under this assumption, we undertook a study for ONR whose objectives were to describe the research-to-application process and to develop potential applications from a representative group of psychological studies.

The first step was to decide which area of psychological research should be the guinea pig. Because a very large amount

*Delivered at the American Psychological Association meetings, New Orleans, August 1974.

of basic research was being performed on human learning at that time, and because of the tremendous social significance of any research-related improvements in training technology, we elected to focus our investigation on the development of applications from studies of learning.

The results of this effort were both disturbing and frustrating (Mackie and Christensen, 1967). After nearly two years of analysis and inquiry, our general findings were that: (1) the great majority of learning research studies (not just those sponsored by ONR) were having virtually no impact on instructional practice in the Navy or elsewhere; and (2) our own efforts to develop applications from the findings of ONR-sponsored research on learning were almost totally unsuccessful. The reasons seem to center, in the first instance, on some seriously neglected activities in the research-to-application process and, in the second, on a number of characteristics of the research studies themselves. I should like to go into these problems in some detail.

Breakdowns in the Research-to-Application Process

Let us first examine a model of the research-to-application process as we assumed it to exist and as we found it in practice. This is shown schematically in Figure 1 which has been coded so as to reflect those elements of the process we found to be active and effectively linked and those which were not. The elements of the process enclosed in heavy dark lines, which indicate very active subsystems, show two highly developed but essentially isolated sets of activities. On the one hand,

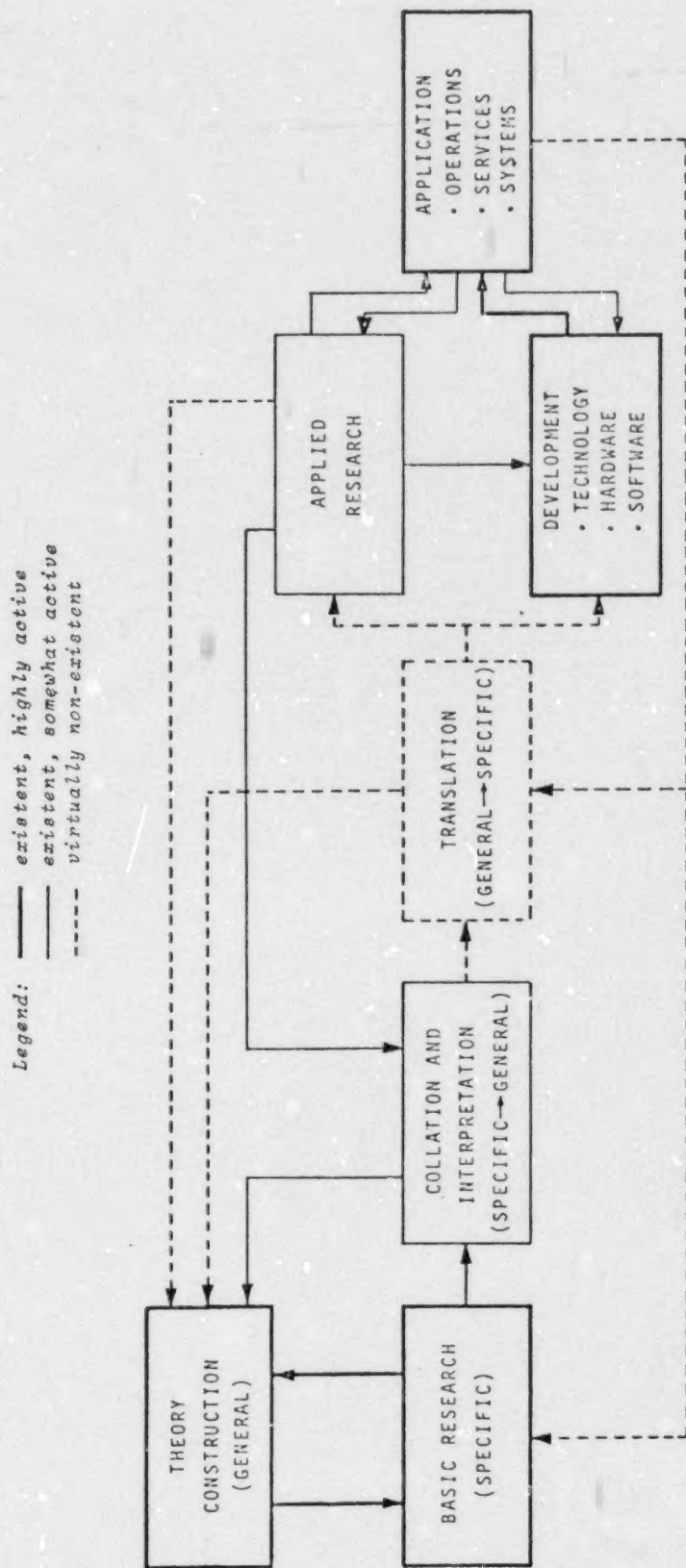


Figure 1. A model of the process of research to application (in theory and in practice).

there is the very active enterprise of basic research and theory construction which takes place mainly in the university laboratories throughout the country. This is typically a closed-loop system with built-in reward systems that rarely demand interactions with the world of practical operations.

At the other extreme is a highly isolated subsystem consisting of applied research, development, and the world of everyday problem solving. Within this subsystem we found, insofar as instructional technology was concerned, that learning research was taking a backseat to the promotion of commercial enterprises such as audiovisual teaching aids, programmed textbooks, and computer-aided instruction. In fact, more of what was going on in the classrooms seemed to reflect the thinking of salesmen for a new educational technology than that of learning researchers. In the past few years, a number of mission-oriented educational research laboratories have emerged that have changed this situation somewhat. Nevertheless, the isolation reflected in the figure between basic research and actual educational operations largely still exists for reasons I will now suggest.

The figure identifies two seriously neglected and under-developed activities that are responsible for the isolation of basic research from application. I have called these functions (1) Collation and Interpretation, and (2) Translation. Collation and Interpretation have their usual meanings here in the sense of critical, comprehensive reviews, together with insightful attempts at generalization, of research results

on some area of human behavior. These functions appear to be increasingly difficult for anyone to perform because of the vast quantity of basic research being produced and its ever-narrowing, specialized nature.

The problem is severe and appears to be growing. In his address at the dedication of the APA building in 1965, Donald Hornig, Director of the Office of Science and Technology and Chairman of the President's Science Advisory Committee, said that the notion that basic research today produces still undefined benefits tomorrow is generally accepted as far as the natural sciences and engineering are concerned. But he emphasized that this trend is not nearly so evident in psychology (Hornig, 1965):

...Despite the fact that the behavioral sciences affect every aspect of our lives and interact with every function of the government, I believe we would all agree that they have not been applied to government problems in the same systematic way as the natural sciences nor has a clear picture emerged as to how their development should be fostered. It is my view that we must move to remedy both these problems. The question is how....

...As the scientific and technical enterprise has grown in scale, increasing concern has developed as to whether the fruits of all this activity, the information derived, is being made promptly available to those who might make use of it....To many the output of science is a collection of facts which can be stored and made available when needed. It is assumed that it will be put to use. But anyone who has ever worked in the sciences is aware that in fact recorded information is often singularly useless,

that much depends on how it is presented, whether written or oral, with what attitude the potential user approaches the information, the degree to which he is stimulated to use that information. And information which is not used in some way helps no one....

I confess that I was surprised to learn that you are one of the largest single publishers of technical journals in the world. Naturally, when one is told that your society publishes more than a million pieces of scientific literature a year one wonders what happens to all of the ideas and facts they contain.

Interpretations of research findings also appear to be increasingly difficult because interpretative statements by the investigator are actively discouraged by many journal editors on the basis of scientific conservatism and shortage of journal space. And, it is a rare individual who is willing to risk interpretation of someone else's research when the original investigator's interpretation is unavailable.

It is also unfortunately true that a great deal more effort seems to be required to produce a creditable review article than to publish a very narrow research study in a specialized area where few critics will be as knowledgeable of the subject matter as the author himself. At least one well-known investigator remarked to us that an invitation to perform a comprehensive review used to be considered a professional honor; nowadays the reviewing function is largely considered a task for one's graduate assistants.

Whatever the reasons, the neglect of interpretative reporting results in a breakdown in the process of developing

the generalizations that appear to be essential if this model of the research-to-application process is valid. It is a necessary step in proceeding from the myriad stimulus, response, and process variables in great numbers of otherwise obscurely related studies to some kind of generalized formulation that in turn can be translated into meaningful operations in a working environment.

This brings us to the next stage in the process, one that we found to be neglected almost to the point of nonexistence. We have elected to call this process Translation. The functions that are associated with this stage in the system have been variously referred to as "bridging the gap," "operationalizing," "social engineering," etc. Whatever the label, the essential activity is one of relating the operations, variables, and functional relationships found to be important in the laboratory to corresponding processes and variables in some operational environment.

The Translation function is also essential to communication, both from the research community to the world of practice and the reverse. We found effective communications between these two subsystems to be almost totally lacking in the field of learning because of the absence of a common technical language, the failure to develop adequately trained personnel to act as intermediaries, and the previously-described neglect of the Collation/Interpretation function. Though we observed this problem in the context of research on human learning, similar problems almost certainly exist in other

areas of psychological research. For example, in his formulation of a proposed research and development model for clinical psychology, Broskowski (1971) has described a role for an individual who "neither does basic research nor dispenses the products of application to the individual consumer." Rather, he is conceptualized as a "man in the middle" who serves as the necessary interface between the basic sciences and applied endeavors by stimulating and mediating interactions among the pure scientists, the product-oriented developer, and the consumer.

It is Broskowski's contention that some Ph.D.-level psychologists must be trained to "systematically conduct and utilize relevant research for the development of procedures and techniques to help solve...various clinical problems in individuals, groups, and institutions." It is his view that the science-professional model on which the training of clinical psychologists traditionally has been based has produced professionals who are either poorly trained scientists or poorly trained clinicians. Instead of emphasizing one or the other end of the process, Broskowski contends that some universities should train specialists to bridge the gap between the poles.

It should be noted, in Figure 1, that the process not only breaks down in going from research to application but also in the reverse process, the feeding back of operational problems from the operational world to the research laboratory. The fact that those who conduct basic research are so thoroughly

isolated from problems in the operational environment is a very significant influence, I feel, on their research designs, which brings us to the second fundamental obstacle in the road from research to application.

Problems Associated with Experimental Procedure

Assuming that the Translation function is a meaningful one, the development of the research translator, social engineer, clinical middleman, or whatever, seems to be an appealing concept. We encountered a difficulty in our study, however, that makes the issue somewhat more complex. The problem was that many research studies seemed not only to defy application but translation as well. The results of psychological research on learning, as well as other behavioral areas, appear to be distressingly specific to the task conditions, independent and procedural variables, and dependent measures selected for use by the investigator. Sensing this specificity, many potential users, rightly or wrongly, are inclined to disregard research whose title does not suggest direct connection with the particular behavior or operation with which they are concerned. Since few research studies meet this stringent criterion, published research articles have an extremely small readership even among those practitioners who presumably should be the most interested (Garvey and Griffith, 1965a, b). Incidentally, I make no distinction in this regard between the utilization of the *results* of a research investigation and its *methodology*, either of which may prove translatable to an operational setting. In fact, psychologists have

contributed more to change in operational systems on the basis of the methods by which they study human behavior than they have in the form of directly useful facts about human behavior.

It seemed to us that the research studies we reviewed not only suffered from specificity of results but also differed greatly with respect to the apparent difficulty a translator would have in deriving applications from them. For convenience, let us assume that research studies vary along some dimension that we will call Application Potential (A/P). If various research studies differ in A/P, in what particulars do they differ? In my view, they differ in a number of highly significant ways that I have attempted to summarize in Figure 2.

First of all, they differ with respect to the basic motivation of the experimenter. Studies with low A/P have as their primary objective the *understanding* of some usually very limited aspect of behavior. In contrast, studies with high A/P have as their objective the desire to *change* something, usually to improve something. The two types of goals are not mutually exclusive, of course. However, there is a matter of primary emphasis that has important consequences for experimental design. Perhaps the most significant of these has to do with the selection of the experimental task.

In low A/P studies, the criteria for selection of the experimental task are typically (1) how well it relates to the experimenter's theoretical position and/or (2) how conveniently it can be generated by the type of equipment that happens to be on the laboratory shelf. In sharp contrast, in studies with

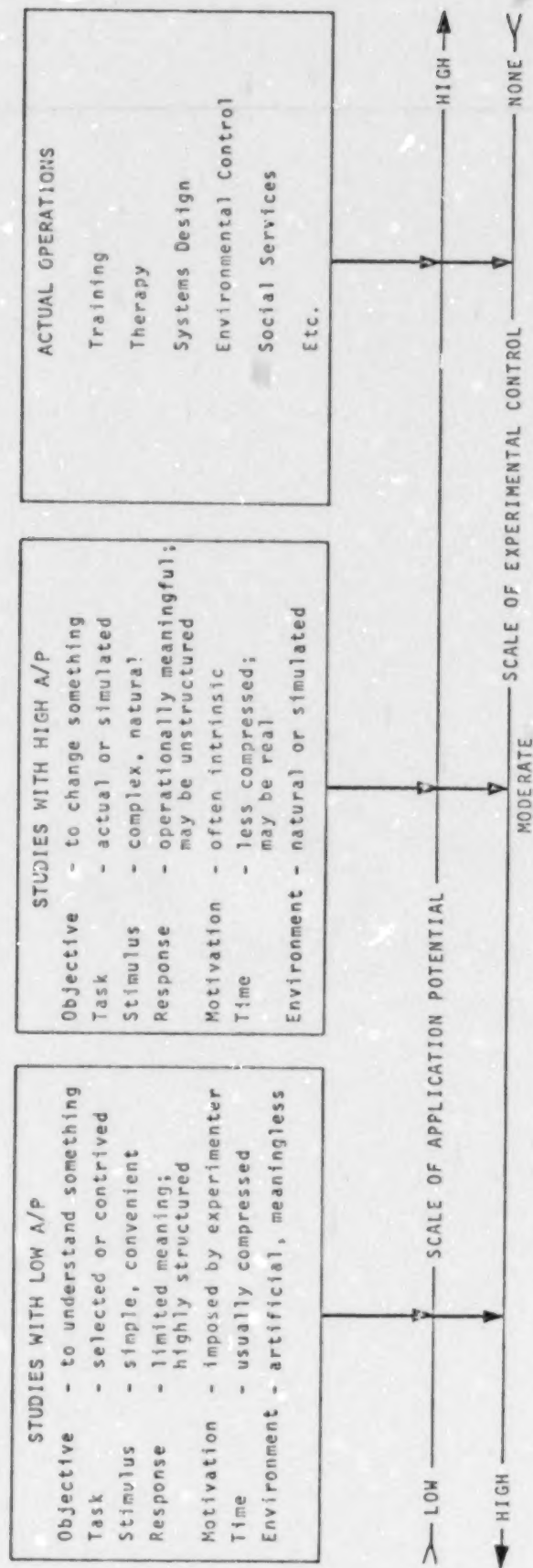


Figure 2. Contrasts between research studies with low and high application potential (A/P).

high A/P, the experimental task is often *imposed* on the experimenter. That is, the starting point for research is often an actual operational task, or part of an operational task, or in some cases a carefully designed simulation of it.

A related way in which studies with low and high A/P differ is in terms of their stimulus and response characteristics. In studies having low A/P, the stimulus tends to be simple, experimentally convenient, and to reflect the interest of good experimental control. The subject's response tends to have meaning only in relation to the experimental task (i.e., "if stimulus A is presented, press the lever marked X as quickly as possible," etc.). In contrast, in high A/P experiments, the stimulus is often *inherent* to the experimental task. By this I mean that the stimulus condition is not contrived by the experimenter but rather is presented in some natural context. The experimenter may have little control of the exact character of the stimulus although he may program it according to some orderly plan. In fact, in many high A/P studies, determining the precise nature of the stimulus may in itself be a significant part of the investigation. For example, in a series of studies that we have conducted into the question of how people learn to perceive and classify complex underwater sounds, it was necessary to perform detailed analyses of the physical properties of the stimuli themselves before we could even begin to understand the nature of the subjects' responses.

The experimental stimuli in a high A/P study may be synthetic and yet be based upon operationally meaningful stimulus conditions. For example, in her excellent studies of how young children learn to read, Gibson (1965) has demonstrated the value of carefully constructed synthetic stimuli that reflect specific hypotheses about perceptual processes. This is in sharp contrast to the stimuli employed in many learning research studies which take the form of nonsense syllables or abstract figures that are devoid of meaning outside of the experimental task itself.

It is not surprising that the use of such contrived stimuli lead to a blind alley insofar as research translation is concerned. There is abundant evidence that the human response to stimulus conditions is very specific; in fact, the entire field of human engineering is built upon the premise that relatively minor modifications in stimulus conditions can result in rather major differences in human response. Examples are also available from other fields, such as the experimental analysis of behavior, which illustrates the requirement for extensive attention to the nature of the stimulus and the contingencies of reinforcement. And, it surely seems that a key element of successful psychotherapy must be the identification of critical stimuli to which new patterns of response must be developed.

This suggests another way in which studies with high and low A/P differ. In low A/P studies, the response repertoire often is very limited and may have no significance outside the

experimental context. In high A/P studies, the response alternatives are typically quite meaningful in relation to some operational criterion. In addition, there may be many response options and not all of these may be fully specified in advance by the experimenter.

Studies with low and high A/P can also differ with respect to the motivation of the participating subjects. In low A/P studies, the motivation is generally *imposed* by the experimenter through some direct appeal to do well, monetary reward, or the hint of extra credit in Psychology 1-A. In high A/P studies, the motivation is usually much more operationally related. Often the experimental objectives are clearly in the interest of the subject himself, and his reward may be a natural by-product of successful performance on the task. This latter condition is most likely to be met if the experimental task is a part of, or clearly relatable to, an operational one which the subject will eventually be called upon to perform in the world outside the laboratory. Neither condition insures high subject motivation, of course, but in the latter circumstances, the experimenter at least has some assurance that he has not introduced a kind of artificial motivation that will fail to transfer to the operational setting.

Still another characteristic on which low and high A/P studies differ is that of time. In Figure 2, I have described the low A/P study as "time compressed" because so many research studies are unfortunately limited to the duration of a classroom period. For some inquiries this may be adequate, but for

studies directed at somehow changing behavior or improving operations, it obviously imposes a severe limit on the probable effects of the experimental variables. In contrast, studies with high A/P tend to be much less time compressed and may in fact occur in real time (although there is rarely *enough* time, especially when interest centers in long-term effects that hopefully bring about change).

A final difference between low and high A/P studies is the difference in the experimental environment itself. In the low A/P case, the experimental environment is typically sterile and deliberately shielded from the influence of variables that are not the focus of attention of the investigator. In the typical high A/P case, the experimental environment is often much more like the operational one or a careful simulation of it. In high A/P studies, therefore, many more uncontrolled variables are likely to influence the results and the effects of the independent variable are likely to be evident only if they are highly influential.

To review, research studies which have high probability of application, which appear to be amenable to translation and application by well-trained behavioral engineers, are different in a variety of ways from studies whose probability of application and ease of translation are very low. These differences are often under the experimenter's control, that is, they represent design options. Since we all probably agree that research utilization is a good thing, the question is, "Why don't we design the majority of research studies in such a way

as to increase the probability of application?" It seems that there are at least two answers. The first is the previously described distance between basic research personnel and the world of operational problems. This is a problem that I feel can and should be corrected through the development of the Translator. We need a new kind of professional to fulfill this role, one who is qualified and comfortable in relating to two different worlds at the same time.

The second problem is more difficult to cope with and it strikes at the very heart of behavioral research. Studies having high application potential are generally characterized by much less experimenter control than studies with low application potential. The risk is that studies designed for high A/P may, because of lack of control, leave many unanswered questions concerning the roles of various variables on the behavioral outcome. On the other hand, restrictions on the nature of the experimental task, the stimulus-response conditions, time, the environment, and the subject's motivation in the interests of experimental control may lead the investigator to a clear understanding of a laboratory phenomenon that, because those very controls were exercised, is difficult if not impossible to relate to operations in the external world.

I certainly have no simple answer to this dilemma. Perhaps we simply must live with a basic conflict brought about by fundamentally different research objectives: to *understand* something versus to *change* something. I personally am more optimistic than this however. In the interest of increased

research utilization, I believe it is possible to train investigators to design studies with considerably higher A/P than is frequently the case without sacrificing the kind of experimental control associated with rigorous scientific inquiry. In my view, the relevance of much basic research is difficult to establish simply because the investigator did not start with an operational problem as the point of departure for his investigation. This is not a trivial matter. A very different experiment is designed if the investigator starts with an analysis of an operational problem rather than with a review of prior basic research or theory alone.

There is clear evidence in the literature (e.g., Gibson, op. cit.) that studies directed at understanding basic behavioral phenomena, that constitute a basis for the elaboration of psychological theory, and that inherently have high application potential, *can* be designed if the researcher adopts *as his point of departure* a thorough understanding of an operationally meaningful behavior problem. This does not mean that the results of such studies will invariably be in a form that can be immediately used by operational personnel. Almost always, the additional step of translation by a behavioral or social engineer will also be required. Thus it is essential that we take seriously the need to develop highly competent professionals who can fulfill these middleman functions. Remember that the requirement exists for greater communications in *both* directions and it may well be that the behavioral engineer must be responsible for a kind of backwards translation,

that is, for describing real-world problems in terms of variables the research community can deal with. But if the research to utilization model has any meaning, if the research translator is to be able to do his job, we must begin to allocate a greater proportion of the research and development effort to studies *designed* to have high application potential. This may imply a reevaluation of the criteria for judging scientific merit in the field of psychology.

REFERENCES

Broskowski, A. Clinical psychology: A research and development model. *Professional Psychology*, 1971, 2, 235-242.

Garvey, W. D. & Griffith, B. C. *Reports of the American Psychological Association's project on scientific information exchange in psychology*, Vol. 1. Washington: American Psychological Association, 1965.

Garvey, W. D. & Griffith, B. C. *Reports of the American Psychological Association's project on scientific information exchange in psychology*, Vol. 2. Washington: American Psychological Association, 1965.

Gibson, E. J. Learning to read. *Science*, 1965, 148, 1066-1072.

Hornig, D. F. Challenges before the behavioral sciences. Paper read at the dedication of the American Psychological Association Headquarters Building, October 16, 1965. Public Information Office, American Psychological Association.

Mackie, R. R., Christensen, P. R. Translation and application of psychological research. *Human Factors Research, Inc., Tech. Rep. 716-1*, 1967.